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LECTURES.

ANEURISM OF THE ABDOMINAL AORTA.

CLINIC OF PROF. ALFRED L. LOOMIS, M. D., MEDICAL DEPARTMENT OF THE UNIVERSITY OF THE CITY OF NEW YORK.[1]

GENTLEMEN: This patient, a man thirty years of age, tells me that he has not been well since three years ago, when he began to be troubled with rheumatism. When I question him more closely, however, I find that he had a previous attack of rheumatism when he was twelve years old, which confined him to bed for two weeks, after which he had constant rheumatic pain in the soles of the feet, knees, and calves of the legs for a year. Some time since he also had some disease about the shoulder-joint, which he says was necrosis, and which was followed by a discharge of several pieces of dead bone. As far as he can remember, he has never received any injury in either that or any other part of his body. He declares positively that he has never had any venereal disease, and I can discover no evidence at all about him of his having been the subject of constitutional syphilis. He says that he can lie down with his head low without any feeling of oppression, and can go up-stairs without inconvenience, except from some pain about the back and hips. When I inquire if there is anything else of which he complains, he replies that he has a swelling in the left side below the ribs, and that he first noticed it two years ago. He is entirely unable to account for its presence, and, as I have previously remarked, does not remember ever having received any injury. There does not seem to be any pain in it, but he has a pain in the back which he thinks is connected with and caused by it. There is one other thing to notice: he says his ankles and the soles of the feet are swollen. [At this point Dr. Loomis called down four students into the amphitheatre, and questioned the first one as follows:]

What should you say, from what you have heard, was probably the matter with this patient? Answer: "Disease of the heart." Why? "From the rheumatic history and the existing swelling of the feet." The patient tells us that the swelling spoken of is simply of a rheumatic

[1] Reported for the JOURNAL.

character: but now in regard to the rheumatic history, would you think an acute attack of rheumatism at the age of twelve more or less likely to be complicated with cardiac disease than one at the age of twenty-five? "Less likely." No, the reverse is true. Heart complications are almost the universal rule in rheumatism at this age, and there is very apt to be pericarditis as well as endocarditis. The patient having now been stripped, you place your hand over the heart and find that the apex beat is pretty nearly in the normal position (though perhaps a little too far to the left), and that it is much stronger than normal. What would you say that this was probably due to? "Hypertrophy of the left ventricle." Now, supposing that on auscultation I found that the first sound of the heart was much longer and more prominent than usual, and yet could detect no cardiac murmur, what would be the natural inference? "That there was hypertrophy due to interference with the circulation somewhere outside of the heart; in the kidneys, for example." Yes, that is so; but in this particular case I think we can exclude kidney trouble, as there is no reason to suspect its presence. [The four students, having made use of the stethoscope, all united in saying that they could hear no murmur.] Well, here is a man who undoubtedly has hypertrophy of the left ventricle, the first sound being very greatly intensified, and yet who has no murmur with it; how shall we account for this state of affairs? It has been correctly stated that hypertrophy may be due either to lesions about the heart itself, or to interference with the circulation somewhere outside; the fact of there being hypertrophy merely showing that the heart is doing more than its normal amount of work. If it is due to some outside interference, this obstruction is much more apt to be found, according to my experience, in the capillary circulation than in the large arteries. A very large aneurism of the thoracic aorta is not infrequently accompanied by a small heart; but a hypertrophied heart is apt to be found with abdominal aneurisms, or those of the lower extremities, as, for instance, popliteal aneurisms.

Now let us turn our attention to the examination of the abdomen. All the four gentlemen say they have found a distinct tumor there, in the umbilical region, and a little to the left of the median line. They describe it as fluctuating, somewhat compressible, dull on percussion (therefore not gaseous), and accompanied by an impulse. Three of them are of the opinion that this impulse is of a dilating, and one that it is of a heaving character. When the stethoscope is applied over the tumor you notice the instrument distinctly rising and falling with it, and on auscultation there is heard a harsh, blowing murmur, synchronous with the first sound of the heart. This sound is not heard so plainly directly over the tumor as upon its right side. When I make palpation myself, I find an evident ovoid tumor, perhaps four

inches in diameter, situated in the median line (and extending somewhat to the left), just below the cœliac axis, and characterized by a very distinct impulse which is dilating and not merely heaving. It is, then, directly upon the line of the abdominal aorta, and must be located either on or in it. Furthermore, it is immovably fixed, and is not tender to the touch. The patient now being upon the hands and knees, we will examine it posteriorly. There is a good deal of pain in the back, extending over a considerable surface, which is not constant (sometimes being entirely absent), and which is aggravated by exercise. On percussion we find an abnormal area of dullness on the left side, corresponding with the situation of the tumor in part, and on auscultation the same murmur as in front, though much less distinct. The intensity of the sound is appreciably increased when I press the anterior abdominal wall up with one hand placed beneath it. The patient says that two years ago he first noticed that there was something unnatural in the abdominal region, though he had little or no pain. Since then he has had a great deal of pain in the back, however, and a continual "sore feeling inside," as he describes it. If he attempts to lift any weight he feels exhausted, his breath growing short and his heart beating violently. He thinks that he is worse than he was six months ago, and that his trouble is increasing every day. On examining the shoulder we find that there does not seem to have been anything the matter with the joint, but that there was disease of the upper part of the humerus, where the pieces of bone came away at two points, according to the patient's account. This may have been due to some slight injury which he does not now remember, but which may have set up a periostitis. The fact that the parts have healed up so completely is presumptive evidence that the bone disease was not due to syphilis.

Now, gentlemen, what is your diagnosis? You all agree that we have here an aneurism of the abdominal aorta. Will one of you please state your reasons for supposing this? Answer: "The presence of a tumor over the abdominal aorta, or on its left side, which is dull on percussion, pulsating in character, and accompanied by a *bruit*." Might not all these symptoms be present and yet there be no aneurism? Yes, they might; but it is rather improbable. Still, there is almost always room for doubt, at least in the opinion of some good observers; and in this connection let me relate a case to you: —

Three years ago a gentleman was sent to me by a physician practicing in the country, in whom I found all the symptoms of abdominal aneurism as clearly marked as in the present instance, while there was a much better history, which seemed fully to account for such a condition being present. Afterwards he was seen by three other medical gentlemen of this city, two of whom were among the oldest and most distinguished men of the profession, while the third, though a young man, was fully

capable of making a diagnosis. Strange to say, both the older men expressed the opinion that there was no aneurism present, but did not state why they thought so. The patient then returned to his home with this conflicting diagnosis. He did not want to believe that I was right, but he followed my advice, which was to keep perfectly quiet and lie down as much as possible. In this he was encouraged by his medical attendant, who agreed with me in the diagnosis of aneurism. In about six months I heard from the physician that he had improved greatly in every way, and that the murmur had disappeared. In six months more he brought the patient to me again, and I found that he was really a great deal better, but made the same diagnosis as before. After that, as I was informed, he got so well that he was apparently as competent to attend to all his business affairs as he had ever been, but in about two years he died very suddenly. There was, unfortunately, no post mortem made, but I think there can be little doubt that aneurism was the cause of his death.

I know of eminent diagnosticians who, if they were to see the case now before us, would be almost certain to give the opinion that it was not one of aneurism. Yet I believe that the man has aneurism, and, if it is not that, I am sure I do not know what it is. The only possible tumor that there could be in this situation would be an enlarged mesenteric gland, but there is no condition, in the present or past history, to account for such a growth. In conclusion, I will briefly recapitulate the reasons for supposing that there is an aneurism here: (1.) The presence of a tumor. (2.) It is dull on percussion. (3.) It is pulsating. (4.) It is on the line of the abdominal aorta. (5.) It is immovable. (6.) It is characterized by a sharp, blowing murmur. (7.) This murmur is heard behind also. (8.) There is more or less constant pain in the back. (9.) There is increased dullness on percussion posteriorly. (10.) There is hypertrophy of the heart. One of the symptoms on which I lay the most stress is the pain in the back, and I have never seen a case of abdominal aneurism in which it was absent. Yet you must not forget that it is possible for all the symptoms enumerated to be present, and no aneurism. It is, therefore, hardly fair to stake our reputation on one examination, and I shall be very much obliged if the patient will return again in about two months.

ESERINE AND PILOCARPINE IN THE TREATMENT OF EYE DISEASE.[1]

BY HENRY W. WILLIAMS, A. M., M. D.,
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It is now about fifteen years since the calabar bean (*Physostigma venenosum*) was introduced to the profession as an agent having the till then unattainable quality of producing at will contraction of the pupil. Its great value was at once recognized by ophthalmologists. But the supply of the remedy, previously unknown to commerce, was limited, and it is only recently that its alkaloids, eserine and physostigmine, have been readily obtainable for therapeutical purposes and physiological experiment.

During the past two years I have made extensive use of eserine in the treatment of corneal ulcers. The great number of cases of ulceration in strumous children and of traumatic and other ulcerations in adults presenting themselves at the ophthalmic department of the Boston City Hospital, together with those occurring in private practice, have afforded abundant opportunities for observation and comparison, and have allowed of an estimate as to the value of treatment which could not be conclusively based on merely a few cases of a disease so variable in its severity and duration.

The modern treatment of ulceration of the cornea as occurring in young children, which had to a great extent superseded the use of caustics, insufflations of calomel, and counter-irritation, has consisted largely in local applications of solutions of atropia. This has been employed to prevent the occurrence of hernia of the iris in case of corneal perforation, and was also and principally used on the theory that it acted as a sedative upon the affected part. As to this sedative influence I have long been skeptical, and unless this can be admitted as an undoubted fact, strong objections exist to the indiscriminate use of atropia. By causing a wide expansion of the pupil and admitting a strong glare of light to the retina it increases the already intense photophobia, and, by thus exciting still further spasmodic contractions, it tends to keep up the morbid processes by the friction and close pressure of the lids upon the ulcerated surface of the cornea, the very thing it is most important to avoid.

It seemed that eserine, by its strong contractile action on the pupil, limiting very much the amount of light which would reach the retina, might lessen the reflex action causing these spasmodic contractions, and thus prove of great advantage. The results of trial have fully justified my anticipations.

[1] Read before the Boston Society for Medical Improvement, January 28, 1878.

In strumous corneal ulceration in children there is little chance that the iris will be involved by contiguity; therefore no objection exists to the use of eserine, so far as any fear might be entertained of closure of the pupil by effused lymph, except where perforation of the cornea has occurred or is imminent. Even then, if the ulcer is at the margin of the cornea, eserine would be indicated, as it would draw the iris away from the perforation and lessen the danger of hernia iridis. If the ulceration is central, eserine may still be used as a curative means, being replaced at any moment by atropia, if desirable, in case perforation is threatened.

Children of tender age can give little direct information as to their sensations, but, judging from their actions and from the repeated testimony of intelligent adults, there is no doubt that a sedative effect, often at least, follows the application of the eserine solution; the supra-orbital pain, which is sometimes one of the physiological sequelæ of its use in a healthy eye, not being felt, but on the contrary a sense of relief from the pain already present in this region.

If we put into the eye a drop of a solution of sulphate of eserine (two grains to an ounce of water) it causes the pupil to contract strongly in about fifteen minutes, and this effect continues for some eight hours. It should be used in the morning, at which time the photophobia is greatest, so that its effect may continue during the day, and may be repeated in the afternoon if required. Its application causes little or no pain. A solution of eight or ten grains of borax to an ounce of water may also be used twice a day, or oftener, as an auxiliary, to lubricate the ulcerated surface and soothe its irritability.

In phlyctenular or herpetic eruptions of the conjunctiva or of the epithelial layer of the cornea, eserine is of service, especially when photophobia is present, and is far preferable to atropia, which by causing intolerance of light adds to the patient's discomfort, and which, also, by exciting spasmodic friction of the lids over the phlyctenular elevations increases the annoying sensation of a foreign body in the eye. There is, unfortunately, a disposition of late, among general practitioners, to employ atropia as a universal remedy in eye affections, probably because so much has been said of its value in iritis.

In traumatic or gonorrhœal ulceration, in ulcerations of the cornea in persons advanced in life or following exhaustive disease, and in creeping ulcer (ulcus serpens) my experience with eserine has been favorable. The circum or supra orbital pain, so often accompanying these ulcers, has been relieved in a marked degree as soon as the remedy had time to act, and the ulceration has assumed a healthier aspect.

I have not yet had an opportunity to employ eserine in the rare but dangerous form of ulcer accompanying some cases of herpes zoster frontalis, but the loss of accommodation and dilatation of the pupil attending this disease would afford especial indications for its use.

In the paralysis of accommodation and mydriasis often resulting from diphtheria and sometimes from measles or scarlatina, eserine is very effective in abbreviating the duration of the abnormal condition. In cases of paralysis of the ciliary branch of the third pair resulting from exposure to cold it is similarly useful. In paralysis of this nerve from traumatic or other causes it is sometimes curative, sometimes only palliative; but even when only the latter, its application, once every day or two, affords much relief in lessening the amount of light, or, in other cases, by reducing the size of the widely dilated pupil gives much satisfaction to the patient from its cosmetic effect. In the hysterical photophobia, which sometimes causes seclusion from light even for years, eserine forms an important part of the treatment.

Having observed a lessening of previously existing injection of the ciliary region after its application (a fact which seems to me important), I should hope for advantage from its use in the commencement of sympathetic irritation of one eye after traumatic injury of the other; but it should be used only as a means of arresting the morbid process after proper measures have been taken for the removal of the source of sympathetic mischief. It, as well as pilocarpine, may be similarly useful in episcleritis. In an instance of extremely conical cornea I have surprised and delighted the patient by the great improvement in vision obtained by the use of eserine.

The obvious effects of the instillation of a drop of a solution of two grains of eserine sulphate in an ounce of water into a healthy eye usually begin to manifest themselves within fifteen minutes. The pupil contracts strongly, becoming, perhaps, not more than a millimetre in diameter; there is often twitching of the lids, and sometimes supra-orbital pain, which, usually slight, may be considerable. Vision is dim, as if the sun were eclipsed. This dimness depends on the narrowness of the pupil, which admits of the passage of only a limited amount of light. There is also spasm of the accommodation, and an induced myopia, which often reaches in a few minutes a very high degree. If this latter is corrected by a concave glass of equivalent power, vision for large objects becomes nearly normal.

These symptoms are usually at their height within an hour, after which they diminish, and at the end of the second hour have in most cases disappeared, with the exception of the contraction of the pupil, which persists for perhaps eight hours or longer.

The above facts are results of my own clinical observation. In the last and the preceding number of Graefe's *Archiv für Ophthalmologie*, vol. xxiii., Parts II. and III., just received, as also in vol. xxii., No. 4, I find accounts of careful and elaborate experiments and observations made by Drs. A. Weber and Mohr, of Darmstadt, Von Reuss, of Vienna, and Professor de Laqueur, of Strasburg, regarding the action of eserine

upon healthy and diseased eyes. These have great value as explaining the *modus operandi* of this medicament, and as affording ground for the belief that it is to prove of extended application in ocular therapeutics, and they confirm in all respects the conclusions I had arrived at.

As regards the effects of eserine upon the cornea, the researches of these gentlemen seem to prove that the activity of the circulation is increased, that the pressure within the anterior chamber is lessened, that the action of accommodation is excited, and that the radius of curvature is shortened during its use. Increased activity in the blood supply, by rendering the cornea more highly vitalized, favors the removal of effete particles and the establishment of a process of repair; the diminished pressure upon the cornea (this pressure being itself a potent cause of ulceration) tends to limit the depth of the ulcer, and lessens the danger of perforation. Dr. von Wecker, of Paris, also believes that eserine prevents the pus from being reproduced in cases of corneal abscess, and in suppuration after cataract operation. We have thus a rational explanation of the benefit derived from the use of eserine in corneal affections.

Dr. Weber considers that the indications for the therapeutic use of extract of calabar bean and its still more efficient alkaloid may be at once deduced from a knowledge of its physiological and, as we may say, mechanical effects. Following these indications in a great number of corneal affections he gives the results, which I translate from his own words: "Calabar has its greatest triumph and its widest application in deep corneal ulceration, and we can assert that the therapeutic value of the means usually employed, such as compressive bandages, warm fomentations, paracentesis, iridectomy, etc., is, with few exceptions, insignificant in comparison with the great efficacy of calabar."

"It appears clearly, from my experiments, that atropine, which is used so generally, and, as I may say, in such a slap-dash manner (*schablonenhaft*), in these affections, increases the infra-corneal pressure to a dangerous degree, and hastens perforation of the corneal ulcer."

Drs. Weber and Laqueur commend the use of eserine, as also of pilocarpine, in glaucoma, not at present, at least, as a substitute for the operative treatment by iridectomy, but as auxiliary means. In their opinion these remedies may arrest the symptoms at the premonitory stage by lessening the intra-ocular tension and relieving the obstructed circulation, and may also prevent a threatened relapse, indicated by a renewal of abnormal tension, after an attack for which iridectomy had been successfully performed.

At the meeting of the Heidelberg Ophthalmologische Gesellschaft in September, 1875, Dr. von Wecker spoke of pilocarpine, the alkaloid of jaborandi, as a myotic, and at the Société de Biologie at Paris, October, 1877, Dr. Galezowski stated that he had found the nitrate of pilo-

carpine, which caused no irritation when applied to the conjunctiva, equally as effective as eserine. His experience was confirmed by Dr. Galippe.

In my own experiments, made with the chlor-hydrate of pilocarpine, the results obtained have differed a little from those produced by eserine sulphate, in the facts that less conjunctival irritation, less supra-orbital pain, and less spasm of the accommodative power seemed to be induced, while the contraction of the pupil and the temporary myopia corresponded in degree with those following the use of eserine. In these respects pilocarpine offers great advantages over eserine. It is, moreover, at present, less costly than eserine, and it does not, as does the latter, deliquesce on keeping.

We have, therefore, unquestionably, two myotic agents capable of rendering immense service in ocular affections, and probably of use in other diseases, especially of the nervous system.

It is needless to say that these, as all other remedies, have their limitations of usefulness; in iritis, for instance, eserine and pilocarpine would doubtless be highly injurious, as tending to congest the already distended vessels, and as favoring the formation of adhesions between the iris and the capsule of the crystalline lens.

THE RELATIONS OF DIPHTHERIA AND "CROUP."

BY T. B. CURTIS, M. D.

In an interesting article upon Diphtheria published in the JOURNAL of January 10th, the question of the relations of diphtheria and "croup" is touched upon, and in this connection a statement is made to the effect that "we have the high authority of Virchow that it [diphtheria] is pathologically distinct from croup." Having already some months ago[1] discussed this question at some length, I hope I may be pardoned if I make an attempt to meet this new argument in favor of the view which I then endeavored to oppose.

The name of Virchow carries with it so much weight that many readers will be disposed to accept his statement as final, since it appears to corroborate, by unimpeachable pathological evidence, the nosological distinction which so many observers are seeking to establish. I am convinced, however, that the distinction drawn by Virchow and the German pathologists is not relevant to the question at issue, which is whether a membranous croup distinct from diphtheria can be shown to exist.

Great confusion has been introduced into this question in consequence of the different meanings which have unfortunately been attached to

[1] See the JOURNAL, July 5, 1877, page 4.

the words diphtheria and croup, diphtheritic and croupous or croupal. To avoid ambiguity, it is therefore necessary that these words should be strictly defined before entering upon any discussion of the significance which should attach to the dictum of Virchow mentioned above.

Diphtheria, then, or cynanche contagiosa, is a disease, specific and infectious, of which the main characteristic consists in the formation of false membranes upon or in certain mucous membranes, and occasionally also upon abraded surfaces, and which is accompanied by a more or less pronounced condition of asthenia, apparently dependent upon blood poisoning.

Croup, better called membranous laryngitis, is an affection whose nosological position is under discussion. According to some authorities, it exists only as a laryngeal localization, primary or secondary, of the specific disease called diphtheria. Others, however, believe it to occur as a simple inflammatory, unspecific, local disease, characterized by the presence of a laryngeal false membrane, and by the symptoms therefrom arising.

With these acceptations the words just defined have a purely nosological signification, and the derived adjectives (diphtheritic and croupous or croupal) are analogous in meaning, denoting that which belongs to or is derived from the respective diseases.

Another wholly different acceptation has been introduced by the German pathologists, according to which these same designations were applied no longer to diseases, but to pathological processes. As so used by Virchow, the words in question have the following meanings: —

Diphtheritic inflammation, or diphtheritis, is a process, inflammatory, exudative, and destructive, consisting in the formation of a morbid product which infiltrates the diseased tissue and causes its necrosis.

Croupous inflammation is a process characterized by the formation of an exudation which is situated not within but upon the diseased part. The exudation when spread out upon a free surface constitutes a croupous membrane; when confined in small spaces or cavities, it takes the form of croupous deposits.

The contrast between the two typical processes just defined is sufficiently manifest, and the authority of Virchow is not needed to enforce it. But does this purely pathological distinction imply a corresponding nosological difference between the diseases diphtheria and croup? Evidently not, unless it can be shown that the diphtheritic and croupous processes belong respectively to the diseases whose names they have been made to bear. So far, however, is this from being the case that in typical cases of diphtheria the exudation is frequently, if not generally, croupous rather than diphtheritic. So often is the exudation of this character that Oertel, describing four forms of diph-

theria, calls one of them the "croupous form," the other three being the catarrhal, the septic, and the gangrenous forms. Niemeyer describes pharyngeal diphtheria under the name of "croup of the pharynx," adding, however, the statement that there is, in this disease, as it were, a transition between the croupous and the diphtheritic inflammations. Other examples of the croupous process, as conceived by German pathologists, are to be found in lobar pneumonia (called in Germany "croupous pneumonia") and in parenchymatous nephritis ("croupous nephritis").

While the morbid process in diphtheria is perhaps more often croupous than diphtheritic, it is in the intestinal lesions of dysentery that "the prototype of diphtheritic inflammation," according to Niemeyer, is to be found. The diphtheritic process also occurs in a typical form in ulcero-membranous stomatitis. This disease is in nowise connected with diphtheria, although consisting in a diphtheritic inflammation of the mucous membrane of the mouth.

Thus we see that although the diphtheritic and croupous processes are pathologically distinct, it does not follow that the diseases bearing similar names are so also.

It has been alleged that the clinical histories of diphtheria and of croup are very different. So, too, is the clinical history of malignant pustule very different from that of carbuncular fever. No one, however, would infer that they were nosologically distinct. It is, on the contrary, universally recognized that these two affections are but separate manifestations of one and the same disease, which may be associated, succeed each other, or exist alone. In diphtheria, also, either the local or the general symptoms may predominate, so as alone to be apparent. The local lesions, generally originating in the pharynx, and often extending secondarily to the larynx, may from the first be restricted to the latter. In a case of primary laryngeal diphtheria (*croup d'emblée*) occurring in a very young child, and progressing rapidly to a fatal termination by asphyxia, the local symptoms due to the false membrane will alone attract attention. Such a case will be liable to receive the name of membranous croup, having, indeed, all the pathological and clinical features attributed to that form of disease.

In the varying degrees of intensity of the local and general manifestations we find a satisfactory explanation of the variations observed in different epidemics of diphtheria. In such epidemic exacerbations as that now in progress, the malignancy of the disease is often extremely marked, and cases which no one can fail to identify as diphtheria predominate. During the intervals separating such outbreaks the general manifestations are apt to be less conspicuous, the local symptoms caused by the laryngeal membrane predominate, and cases having the characters attributed to "croup" become more common. Similar variations, quite as

pronounced, are observed in all the infectious diseases which occur epidemically, malignant forms of small-pox or of scarlet fever, for instance, differing very considerably from ordinary mild forms of those diseases.

The object of my argument is not, by any means, to assert that the unity of the pseudo-membranous disease is conclusively demonstrated, but to maintain that all attempts to prove its duality have thus far been unsuccessful, whether on pathological or on clinical grounds. It is not impossible that at some future day diphtheria may be divided by nosological distinctions based upon a more accurate knowledge than we as yet possess of the *materies morbi* or of the ætiological conditions under which the disease arises. I am, however, convinced that no criterion, pathological or clinical, has thus far been shown to exist by which we can trace any such line of demarcation as is held to separate croup from diphtheria.

Finally, even if the view which I am opposing should prevail, if it should be maintained that a non-diphtheritic, *non-transmissible* croup exists, every one must, I think, admit that the difficulty of making a correct diagnosis between such a form of disease and certain cases of primary laryngeal diphtheria must be well-nigh insuperable. It would therefore be highly rash to rely upon such a diagnosis to the extent of allowing the precautions considered necessary in cases of diphtheria to be neglected.

For this reason, it seems to me, that in the order of the city Board of Health enjoining upon householders and physicians to report cases of infectious disease, "membranous croup" ought unquestionably to be placed upon the same footing as diphtheria. It is comparatively unimportant, after all, under what names cases are recorded, provided that we are careful to keep on the safe side in matters relating to their practical management.

RECENT PROGRESS IN ANATOMY.[1]

BY THOMAS DWIGHT, M. D.

Lymphatics. — Among the contributions to our knowledge of the lymphatics we must notice the papers by George Hoggan, M. B., and Frances Elizabeth Hoggan, M. D., presented to the Royal Society of London, of which we have as yet only the abstract.[2] They claim to have discovered the lymphatics of striped muscular fibre. These consist of radicles, reservoirs without valves, and efferent vessels with them. The reservoirs are found on one side of a muscle and the efferent vessels on the other; this is the state in the diaphragm, for instance. We regret that we do not find any account of the radicles. The efferent

[1] Concluded from page 309.

[2] Proceedings of the Royal Society of London. Vol. xxvi., Nos. 178 and 182.

vessels are on the thoracic side of the diaphragm and on the outer side of the abdominal muscles. The authors believe that the lower surface of the diaphragm is an exuding one, and absorbent only under abnormal conditions. They admit the existence of st mata in the serous cavities of the frog, but deny that they are found in mammalia, or that the serous cavities of this class of animals can be considered parts of the lymphatic system.

The same observers have studied also the lymphatics of the skin, and we will now quote the abstract itself: —

"For the purpose of anatomical description they divide these lymphatics into three categories, named from their position the subhypodermic, the dermic, and the subepidermic. Only the first and third can be described as layers; the second consists of horizontal and vertical sets of vessels, extending through the whole thickness of the dermis and connecting the other two distinct layers together. All the lymphatics of the hypodermis, and most of those of the dermis, are valved efferent vessels, without any collecting channels that would entitle them to claim any absorbing function in these portions of the skin through which they merely pass.

"The subepidermic lymphatics are narrow parallel collecting channels, destitute of valves, lying, as their name imp ies, immediately under the epidermic cells in young animals, though separated from them, as adult life is reached, by bundles of gelatinous tissue. These are the only radicles of the lymphatics of the skin.

"Upon the subepidermic lymphatics they find a rich plexus formed by multipolar nerve cells and non-medullated nerve fibres, the distribution of which to the epidermis has been made evident by the same process. . . . . Neither sweat glands, sebaceous glands, hair muscles, fat cells, nor nerve bundles possess any lymphatics, and the papillæ of the human skin are equally destitute of them. Functionally, the lymphatics of the skin are to be considered as forming two classes: the valved efferent vessels with independent walls, formed only of crenated endothelium cells, and the valveless collecting channels of the subepidermis, lined by those crenated cells.

"Upon the facts accumulated in this and their former paper the authors are led entirely to reject the theory of vasa serosa or radicles of the lymphatics, formed by chains of connective tissue cells or the cavities in which they lie. In the human skin especially these cells of the connective tissue are numerous and in intimate relationship with the superficial blood-vessels, but prominently absent from the collecting lymphatic channels lying alongside of these vessels, thus supporting the hypothesis they formerly emitted, that these cells were merely links in a nutritive chain, not radicles of the lymphatics, even when, as in tendon, the cornea, etc., they are connected with the lymphatics."

The above statements are of sufficient importance to deserve reproduction, but without seeing the illustrations which will appear with the papers when published at length, we can express no certain opinion as to their correctness. We have, however, looked on the stomata of serous membranes in mammalia as so well demonstrated that we shall be slow to give them up. It is remarkable that the authors make no mention (in the abstract at least) of the fat canals described by Dr. Warren.[1]

The Suspensory Ligaments of the Diaphragm.[2] — That the under surface of the pericardium is closely united with the tendinous centre of the diaphragm and must oppose its descent into the abdomen is certainly not new, but Dr. E. v. Teutleben has described more thoroughly than has been done the series of fibres by which the centre of the diaphragm is restrained, and has traced them to their origin from the spinal column. It may be as well to reproduce a passage from an article by Béraud which v. Teutleben quotes, but with which he was not acquainted when he began his work. It is as follows: "The uses of this superior ligament of the pericardium appear to me to deserve the attention of physiologists. It holds the pericardium firmly and prevents its descent, so that the name of 'hollow tendon of the diaphragm,' which MM. Beau and Maissiat gave to the pericardium, appears to us perfectly justified. In fact, by means of this insertion, the diaphragm has a solid point of support from the pericardium, which as its descent is limited will prevent too great a displacement of the partition between the thorax and abdomen." As described by v. Teutleben the ligaments of both sides may be divided into an upper and a lower part. The latter consists of a strip of fascia springing from the tendinous centre of the diaphragm lying on the pericardium and reaching the root of the lung. The upper part arises from the last cervical and first dorsal vertebræ, sometimes from several more, and as it descends it splits into two layers, of which the more superficial surrounds the great vessels and passes into the pericardium, while the deeper goes to the trachea and root of the lung, sending, however, fibres into the pericardium. Thus it appears that the functions of these ligaments are not only to hold up the centre of the diaphragm, but to support the thoracic viscera and protect them from too great motion.

Persistence of Fœtal Blood-Vessels. — The appearance in advanced stages of cirrhosis of the liver of a number of dilated veins on the abdomen radiating from the navel has long been called the *caput Medusæ*, and in old times was accounted for by the theory that the umbilical vein of fœtal life had been reopened by the pressure of the blood. That a large vein was found in the round ligament in such cases was certain,

[1] Journal, April 19, 1877.

[2] Archiv für Anatomie und Entwickelungsgeschichte, 1877, Heft 3 and 4.

and it was taken for granted that it was the old umbilical vein till 1859, when Sappey asserted that it was impossible for a solid fibrous cord to resume the functions and structure of a vein, and that the enlarged vein, which, to be sure, ran beside the obliterated umbilical vein, was an accessory portal vein which opened into the left branch of the great portal vein. This view has been very generally accepted, but Dr. Baumgarten [1] has recently made some researches which he thinks prove that the old view is the correct one. He states that if a bristle is passed into the vein mentioned by Sappey at its opening into the left branch of the portal vein, and then microscopic transverse sections made, we find that the bristle is inside of the old umbilical vein, which has been partially obliterated by a growth of connective tissue around it. The structure of the walls of the umbilical vein is easily distinguished from ordinary subperitoneal veins by the typical arrangement of the muscular fibre in its walls. The cavity, lined with endothelium, can be distinctly made out to near the abdominal wall, and later the remains of the closed vessel to the end. Sometimes the vessel is large enough to admit a probe for an inch or two. The vessel, as a rule, contains fluid blood, which reaches it by small veins. Pathological cases of enlargement of the vein have confirmed Baumgarten in his opinion that it is really the umbilical. He has made observations, which, however, are not yet completed, on the ductus venosus, which he finds as yet in every instance to contain a small cavity filled with normal blood corpuscles. Whether in all cases this keeps open, though on a very small scale, the direct passage from the portal to the hepatic vein is, we think, still uncertain.

PROCEEDINGS OF THE SUFFOLK DISTRICT MEDICAL SOCIETY.

A. L. MASON, M. D., SECRETARY.

NOVEMBER 24, 1877. Eighty members were present, DR. HOMANS, the president, in the chair.

Report of Committee. — The committee appointed to remonstrate against the reopening of Jourdain's "Anatomical Museum" reported that they had appeared before the licensing committee of the city board of aldermen, and that the case had been presented in full at a meeting of the Natural History Society. The result was that no license was granted.

Water-Closets. — DR. N. FOLSOM showed an improved water-closet bowl, suited for any pan-closet, described by him in an essay on Hospital Construction written for the Johns Hopkins Hospital, and now made by Harrison & Co., 16 West Fourth Street, New York. The upper rim of the bowl is expanded into a shallow sink or drip tray eighteen inches square, which is fully displayed on lifting the ordinary seat, and which replaces the sheathing

[1] Centralblatt für die medicinischen Wissenschaften, October 6, 1877.

of lead which is commonly used. The incrustation of offensive material usually found upon and under the lead is thus avoided, and every part can be readily and thoroughly cleaned and made odorless. It is intended as a slop-sink, and to replace the ordinary urinal, which is a source of offense from the collection of urinary salts at inaccessible points, and from exhalation from the portion of pipe between the strainer and the trap below. The woodwork should slope in, in front of the water-closet at the floor, about three inches, to give room for the feet of the person standing before it.

Domestics often do not know that a water-closet bowl should be washed throughout as frequently as a hand-basin. Hot water and soap and sometimes a little sand are necessary to remove urinary incrustation, which will deposit in spite of stream of cold water.

Public attention has recently been called to the necessity of carrying the soil-pipe up through the roof above to prevent the regurgitation of sewer gas. In pan-closets there should also be a vent-pipe leading from the receptacle below the pan up to the roof. This pipe may be conducted into the soil-pipe above, but this method is open to the objection that it invalidates the trap beneath it. The regurgitation of offensive air when the handle is raised is avoided by either arrangement. The Jennings closet and others are advocated as doing away with this receptacle below the pan, but if this space is furnished with a vent the objection to it vanishes, and the pan-closet seems likely to retain its reputation of superiority as being less liable to get out of order, while it can be rendered absolutely inodorous by proper arrangement. The pan is now made with porcelain lining. The Jennings closet was tried at the Massachusetts General Hospital, but did not prove satisfactory. An incident showing its defective action was the finding of portions of fæces tainting the air by floating in the exposed surface of water above the "plug" which closes the outlet. For hospitals, where it is uncertain what may be thrown in, a "hopper" closet without pan or receptacle is preferable, the deposit dropping directly into an S trap flushed with a very large and sudden supply of water both by moving a handle and automatically by opening the door.

A source of offense during and after the use of a closet is the gas which passes from the bowel and the immediate odor of the fæces. Ventilation of the room is necessary, and a window is *not* the best means. There are few closets so situated that a tin pipe cannot be carried from below the seat to a chimney. If this be the kitchen chimney a draught is certain, but if warm air is supplied to the closet by a register or by an opening from a room well supplied, and *there is no other exit*, the draught is equally secure. A *supply* is needed in any case. Frequently a ventilator is placed in the wall or ceiling

of a water-closet, but the proper way is to seize the odors *where they arise*. The seat should rest on cleats, leaving an inch of space always open above the bowl, connecting with the tin pipe behind, the draught being horizontal from front to back.

Dr. Ellis said that if the valve of the Jennings closet became in the least out of order the water would be drained from the largest cistern. This had happened at the Massachusetts General Hospital.

Dr. Bowditch had found the Jennings closet satisfactory, and much neater in appearance than the ordinary closet. Care was necessary in shutting the valve.

Dr. Ayer inquired with regard to the efficacy of the usual tin ventilating pipes communicating with chimneys.

Dr. Folsom thought that earthenware or lead was better, as there was no corrosion. A register or space below the door to admit air was essential.

Disease of Knee. — Dr. M. E. Webb reported a case of disease of the knee-joint, and showed the specimen. The case is reserved by the author for publication.

Naso-Pharyngeal Polypus. — Dr. Cheever presented a patient from whom he had removed a large naso-pharyngeal polypus by depressing the nasal bones. There was complete recovery, with perfect union and natural voice, in three weeks. Dr. Cheever will publish a full report of the case.

Intestinal Cast. — Dr. Wing showed a cast of the intestine, a yard and a quarter long, which had been passed by a woman who had suffered for a few days from colic, to relieve which laudanum had been used. There had been no diarrhœa, and the cast was evacuated without pain.

Dr. A. N. Blodgett, who had examined the tube microscopically, said that it presented the appearance of a mucous exudation, the previous cellular wall having undergone fatty degeneration. It might easily, on hasty inspection, have been taken for part of the intestinal canal.

Dr. Wing mentioned a similar case, in which the tube had been regarded as a portion of the bowel, and an unfavorable prognosis had been given.

Dr. Jackson spoke of the diarrhœa tubularis of Good, and said that he had seen a few such cases himself. He regarded the exudation as similar to that which sometimes takes place in the bronchial tubes.

Dr. N. Folsom said that he had expelled mucous casts from his own throat, of arborescent form, and exactly similar in appearance to the fibrinous casts.

Artificial Limbs. — Mr. James W. Drake exhibited to the society specimens of his improved artificial limbs.

Pneumono-Dynamics. — Dr. D. Hunt read the following criticism on Dr. Garland's paper on Pneumono-Dynamics: —

While in conversation with Dr. Garland, after the adjournment of the last meeting of the society, I informed him that I disagreed with the opinions that he had just presented;[1] this led to a pleasant discussion and a proposition on my part to review his theories before this society. Dr. Garland consented, but I had no sooner commenced the arrangement of a line of argument for demonstrating my opinion than I was sorry that he had done so, for I found myself com-

[1] See Journal November 29, 1877.

pelled to state some mistakes of his that concerned very elementary facts. It was, then, a great relief to me when, a few days later, Dr. Garland called upon me, and stated that he had found some mistakes in his interpretation of certain experiments, and requested me not to notice his paper before this society; to this I readily agreed. I was surprised, therefore, when, last week, he repeated his invitation for me to criticise his views at this meeting. Having expressed myself quite forcibly as to his errors, I knew of no way of refusing without leaving a false impression in his mind. I make this statement at some length, since these views of Dr. Garland, having received the public sanction of an authority so eminent as Professor Ellis, and being embodied in a work now almost through the press, might well be left to that public judgment that in the end decides the value of all new opinions. In appearing to-night in anticipation of such public judgment, it will be understood that the occasion is not one of my own seeking.

My thanks are due to Dr. Garland for the advance sheets of his forthcoming work, as far as they concern his theory and the experiments by which he supports it.

I translate from Hermann's Physiologie des Menschen the following account of the physiology of respiration: "The lungs in the body are inclosed in a rigid reservoir of great volume (thorax) in such a manner that between their outer surface and the inner surface of the containing cavity, or, more exactly speaking, between their pleural covering and the pleural lining of the thorax, there is no air, and none can enter. The pressure of the atmosphere must therefore distend them in opposition to their elasticity beyond their natural volume, so that they are everywhere in immediate contact with the wall of the thorax, and are thus during life always filled with air; if air enters into this space between lung and thorax wall, the lungs collapse by means of their elasticity.

"Not only lungs, but heart and blood-vessels contribute to fill the space of the thorax; atmospheric pressure is working upon the inner surface of all these organs: upon the lungs directly (by communication through trachea, etc.), upon the heart indirectly, since the whole body, and consequently the blood-vessels without the thorax, are subject to atmospheric pressure and communicate it to the contents of the heart. As the same pressure is thus developed in all the hollow organs in the thorax, these are stretched in proportion to their elasticity: the most easily distended organs, the lungs, thus contribute most toward filling the thorax, are most stretched beyond their normal volume, the thick-walled ventricles of the heart the least (scarce perceptibly), the thinner-walled auricles and venous trunks to a greater degree. The yielding portion of the wall of the thorax upon whose outer surface atmospheric pressure is working must also by bending inward contribute somewhat towards filling or rather lessening the thoracic cavity. Thus diaphragm and intercostal soft parts are arched into the thorax."

A little farther on Hermann says: "The elastic power with which the lungs, distended to fill the thorax, endeavor to attain their natural volume, that is, their negative pressure in the thorax at rest, may be measured by fixing a manometer air-tight in the trachea of the subject, and then opening the thorax; it amounts to about six millimetres of mercury (Donders)."

Notice that the negative pressure of the lungs does not become a positive or working power until the thorax is opened and atmospheric pressure acts alike upon outer and inner surface; until this equilibrium is established the lung gets its six-millimetre negative power by being distended by a seven-hundred-and-sixty-millimetre positive power (atmospheric pressure).

Dr. Garland's first proposition, in 1874, was "the lungs seem to be the molding agent" in case of effusion into the thorax; "they in some manner resist the encroachments of the fluid." "The irregularities of the upper part of the model were explained by the supposition that the fluid spurted up in parts where the pulmonary resistance was least." But he goes on to say, "Mature deliberation, however, convinced me that the conclusions were incorrect, because the lung is a non-resisting body, and therefore cannot exert counter-pressure." (Pages 28 to 30.) If the lungs are non-resisting, they they are the first non-resisting solid that I have heard of.

Dr. Ferber, it seems, could not satisfy himself with such facility of this character of the lungs, so he proceeded to revise Garland's experiments that he might by observation determine the rôle played by the lung in influencing an effusion into the thorax.

(1.) He states the necessity of making experiments upon living, breathing animals, since the distribution of the exudation cannot depend upon either gravity or lung elasticity alone, but must be also dependent upon the movements of the lung as a whole. (2.) A substance must be chosen that solidifies slowly, that there may be time for a proper distribution of the fluid; to fulfill this indication he chose cocoa butter. (3.) The animal must be kept alive fifteen minutes without having spasms or convulsions. (4.) Injections must be made with the animal in different positions, that the results may be of value as proof. (5.) The material must be introduced in a manner that will avoid all strong pressure, for by this means local atelectasis may be produced. (6.) Air must not be admitted. (7.) The most important experiment of all to Ferber, the all-controlling one (page 35), was dissecting away skin and muscles, and thus making a window of the costal pleura, through which he could watch the movements of the lungs and the fluid in the pleural cavity.

With all these conditions in mind he proceeds to answer this question, What forces are effective in shaping and bounding the exudation not restrained by adhesions? (Page 36.)

He then made injections, under these conditions, which Dr. Garland afterwards adopted in his experiments; as a natural consequence, Ferber's and Garland's models are essentially alike; the question of their interpretation is that which occupies us this evening.

Ferber concluded as follows from the study of his models and from watching the action of the injected fluid through the window which he made in the wall of the thorax: —

"The position of the exudation as a whole is undoubtedly chiefly caused by the gravity of the exudation itself and the position occupied by the animal experimented on." (Page 36.)

Garland's idea of 1874 is disproved by Ferber by the following arguments, which Garland has never answered: —

"The inner surface of the injection, that is, the impression of the compressed lung in the model, represents, to a certain extent, but in lessened proportions, the form of the normal lung. That this lessening is not caused by a lung retracting equally in all directions is shown not only by the position of the model, but also by the unequal thickness of one and the same model in different places. If the lung retracted equally, or, in other words, if the distribution of the exudation depended upon the resistance of the equally retracting lung, then the fluid must from the beginning spread itself in nearly equal thickness all around the lungs; that this is not the case is proved by the model."

Thus Dr. Ferber disproves the idea that the lung is the distributing power by showing that if it were we should get a cast, in the model, of the almost symmetrically contracting lung. In passing, let me say that Dr. Garland appears to be in error in regard to the anatomy of the lungs; he says (page 62): "From the application of this principle of the interpretation of our models, and from our knowledge of the anatomy of the lung, we now know that the lower part of the lung, being most distended, attracts to itself fluid to the deprivation of parts less energetic."

Ranke's Physiologie (3te Auf. page 451) speaks on this point as follows: "S. Stern concludes that during inspiration a certain inequality in the distention of the lungs occurs which is the more considerable the greater the activity of the thoracic walls. In general the *superior lobes are more distended than the inferior*, and especially in the neighborhood of their anterior border. The reason for this is that the resistance of the distended lung is not able to change the form of the rigid or nearly rigid wall of the thorax, and the unequal distentions effected by the wall of the thorax itself can be only partly canceled by the accompanying action of the diaphragm."

I can find no explanation of Dr. Garland's present position, excepting that he has not fully comprehended the nature of the arguments with which Ferber upset the views which he put forth in 1874. If he had, I do not believe that he would state his present theory of pneumono-dynamics, for by this theory he imagines that the lungs actually suspend the column of fluid by virtue of their retractile power; but the retractile power depends upon the degree of distention of the lung, which is all the time decreasing while the amount of fluid is all the time increasing; that is, this power of the lung, which is being obliterated by the encroachment of the fluid upon the space which is essential to its development, is doing a continually increasing amount of work in sustaining an increasing weight of fluid.

We will quote from Dr. Garland's book his translation of Dr. Ferber's account of two of his models, together with his comments on Ferber's description: —

"Model I. Obtained with the dog lying upon his back. The fluid lay along the vertebral column, and showed *no depression* of the diaphragm. The external upper border presented slight pointed projections and ran *nearly* parallel with the axillary line, that is, nearly horizontal. Injection through ninth intercostal space in the axillary line of the right side. Large dog."

Having thus translated Ferber, Dr. Garland comments as follows: —

"The italics in this and in the other quotations of this chapter are my own. Now a fluid whose upper edge runs 'nearly' horizontal, and presents even slight pointed projections, can scarcely be said to have assumed a hydrostatic level. No fluid can change its level voluntarily. However slight may be the deviation from a horizontal level, that deviation indicates the interference of some external agency."

It will be seen that Dr. Garland implies in this comment that Dr. Ferber says that the surface of the fluid assumes a hydrostatic level; this is doing great injustice to Dr. Ferber. I have searched his monograph thoroughly, and can say that he states nothing of the kind. It is Dr. Garland's idea of a hydrostatic level that is at fault; he supposes that when Ferber says "nearly horizontal" he means nearly a hydrostatic level, but if Dr. Garland had thought of the very elements of hydrostatics he would not have spoken of such a level as appertaining to a fluid which is bounded on every surface by solids; the respect due to his cultured antagonist, who is in this very monograph writing for his "*venia docendi*," should have restrained him from imputing such crass ignorance to him. But Dr. Garland not only misinterprets Ferber upon this point, indeed his whole book is based upon just this misapprehension.

Dr. Garland thus translates Ferber's description of his fourth model: "Injection in the axillary line of the right side, ninth intercostal space. Position perpendicular upon head. The exudation sat like a hood upon the apex of the right lung, and was limited by a superior horizontal line."

Dr. Garland comments as follows: "I made no injection under the condition named with the model, but the results obtained are precisely what I should expect from the principles which I have already explained. The apex of the lung is not concave like the base, but it is convex; hence the mutual adjustments of apex and injection coincide with those of balloon and water in Experiment I., Chapter III. If the water in the flask should solidify, the model would appear like a hood upon the balloon. Moreover, its upper border is horizontal because the line of cleavage between the balloon and the glass is horizontal, and not because the specific gravity of the water places it so. The superior surface of the water, however,—and this is the striking feature of the experiment,—is *concave*. The superior surface of Ferber's model was also concave, because Ferber says it rested upon the apex *like a hood*. How can a fluid whose superior surface is concave be in a state of stable equilibrium?"

Dr. Garland imputes to Ferber the absurdity of supposing that a hydrostatic level exists; in this instance as in the former it is Dr. Garland's misunderstanding of the definition of a hydrostatic level, and his interpretation of Ferber by this mistaken idea, that is at fault. A perpendicular to a hydrostatic level is always directed toward the centre of the earth, but a perpendicular to a horizontal surface of water that is confined, that is not free, is merely an axis of the vessel which contains the fluid.

But let us examine Dr. Garland's models and their interpretation by his theory that the lifting power or elasticity of the lungs raises columns of fluid above the hydrostatic level of such fluid,—in other words, his problem of pneumono-dynamics.

"Model II. The merest glance at the model will be sufficient to show that the line C D represents the imaginary hydrostatic level of all fluid below it. I speak of fluid, because it will be remembered that at the time the adjustments represented in the model were made the cocoa butter was in a fluid state. The youngest child who had studied the very elements of hydrostatics would perceive that the body of fluid above C D does not present a hydrostatic level. On the contrary, the perpendicular lines x y, x' y', x'' y'', x''' y''', etc., represent columns of fluid, which, by some agency or other, are elevated above the level which their specific gravity gives them, and the sum of all these lines represents a body of fluid sustained by some invisible agency above its natural level. Let us see if we obtain the same condition of affairs in our other models." (Page 24.)

Thus, although every imaginable layer of the body of fluid above C D has the most solid basis, resting as it does on the layer below it, Dr. Garland considers it as "sustained by some invisible agency above its natural level."

"Model III. Dog inclined at an angle of about 45°. Large injection in ninth intercostal space of the left side. A thin portion of the model, corresponding to the small space A B D E was destroyed in removing the mass from the chest. Now if we draw the line F D E G we shall represent the hydrostatic level of the body of fluid below it. The two masses above that line, however, F K A and E M G are evidently in a state of inequilibrium which no principle of hydrostatics can explain. The blank space above K A B M was occupied by the lung, and the latter was still in contact with the chest wall." (Page 26.)

Here is a still more striking evidence of Dr. Garland's confusion as to what constitutes a hydrostatic level; he runs an imaginary horizontal plane through a body of liquid inclosed in a given space, and against all laws of physics considers such a plane as representing a hydrostatic level, and all fluid above it in a state of inequilibrium. Fluids shut in on all sides have no hydrostatic level; such a level is only possible in an open or a partly filled closed vessel.

Dr. Garland proceeds: "Model IV. Dog horizontal; very large injection. Here we have once more the same condition of affairs. Horizontal line A D represents the hydrostatic level of all the fluid above it, but all the fluid above this line seems to have no possible means of support. B M N C is a large surface of the lung which was still in contact with the chest wall. Now the question arises, What force supports the two columns K L and H I? If we have two flasks communicating at their bases by a tube, and we pour water into one flask, it will pass through the connecting tube until it stands at the same level in the second flask. One might say, therefore, that the columns K L and H I communicate at their base through the vertebral groove, and that they thus balance each other, though they cannot stand at just the same level because of the slope of the chest wall. This explanation will not apply to our model, however, because if the two columns K L and H I were in a state of mutual balance the columns E P and F R ought to be similarly related to those columns, since they also communicate at their base with the fluid in the vertebral groove."

Dr. Garland comments as follows: "We see, therefore, that one constant

phenomenon is exhibited in all our casts. Large bodies of fluid are supported above their hydrostatic level by some agency not yet discovered. No principle of hydrostatics will explain this phenomenon. No change of position can effect it, since the condition of hydrostatic inequilibrium is constant in every position which the animal assumes."

Dr. Garland says "no agency is yet discovered" to account for, and that "no principle of hydrostatics will explain this phenomenon;" the latter statement is true, for the problem does not concern hydrostatics at all. Hydrostatics is that part of science that treats of fluids at rest; but a fluid exerting a pressure in every direction in its effort to reach a stable equilibrium, the effects of whose pressure we see in bulging thorax walls, a dependent diaphragm, and a collapsed lung, can hardly be said to be at rest. If, however, he should make a careful application of the laws of the action of gravity to his problems he will find a key to their solution which will relieve him of the task of discovering a new law or principle in the pretty well cultivated domain of hydrostatics.

Dr. Garland might say that he merely refers to imaginary hydrostatic levels; if this is the case it follows, of course, that the conditions of inequilibrium and the forces creating it are also imaginary, for these exist only in virtue of the existence of the hydrostatic level.

To repeat our objections, the fluid must have a free surface; that is, it must be exposed to air, or gas, or a vacuum, in order that it may have a hydrostatic level. There is no air in the pleural cavity, and none can enter; the same applies to gas, unless such gas results from decomposition; it would, of course, be needless to think of any retractile power of lung in such a case. As to a vacuum, if the lung is lifting a column of fluid, it lifts just as a pump lifts; but such a force will lift for thirty feet, and no vacuum can be formed over fluid lifted until after this point is passed. No thorax is thirty feet high, consequently, no such vacuum is possible. We have thus exhausted all the conditions under which such a level is possible in the thorax, and find that none apply to the pleural cavity; therefore, the fluid in such a cavity cannot assume a hydrostatic level, and if no such level is possible no fluid can be said to be lifted from it.

To recapitulate. Dr. Garland states that—

(1.) The lung is a non-resisting body; but we know that it is a solid, having form and structure. It is, then, a resisting body. This requires no proof.

(2.) He assumes a hydrostatic level in a fluid which is bounded on all sides by solids. This all physicists will admit is against every principle of physics.

(3.) The experiments were so crude, considering their object, that it is impossible to determine whether the lung at the time the cast was made was exercising any retractile force whatever. The experiments, as described, fail to show that at the time the cast was formed (that is, at the time the cocoa butter solidified) the lung was exercising any "retractile" force. For although investigating a force represented, at best, by a column of mercury only a few millimetres high, Dr. Garland has omitted to notice, on the one hand, the pressure of the atmosphere (that is, barometric height), a force represented by more than seven hundred millimetres of mercury, and tending to resist com-

pression of the lung; while on the other hand he does not state the compressing force, namely, the force with which the injection was introduced. Now we know by the experiment of Donders that the lungs when *fully expanded* against the walls of the thorax tend to contract with a force represented by six millimetres of mercury; we know also that when air is admitted into the thorax, or when the lungs are removed from the body, they contract to a certain volume and then have no farther retractile power; again, subject the lungs to any external pressure greater than that of the atmosphere and they will be compressed, and will then exercise an expansive rather than a retractile power.

Thus Dr. Garland's experiments are failures as means of demonstrating his theory, since he gives us no observed facts by which we can determine by physical laws whether the action of the lung was retractile (lung distended beyond its natural volume), inactive (lung at normal volume), or expansive (lung less than normal volume) at the moment when the casts were formed.

(4.) Under known physical laws the cast would assume no other form than that which it did assume. For the liquid as it enters, whether introduced by atmospheric pressure, effusion, or the syringe, makes a cavity for itself by displacing the material offering the least resistance; the lung gives way and the fluid enters. As the liquid is now bounded on all sides by solids it is physically impossible that it should assume a "hydrostatic level," but it must conform to the walls which inclose it. (See Proposition 2.)

(5.) The change of form of the cast caused by injection in the different positions of the body is the effect purely of the relative specific gravities of the solid lung and the liquid injection. We can show this by means of this bottle containing water and a bit of cork which is fastened by thread to the cork stopping the mouth of the bottle. If the cavity of the thorax contains a fluid (water, melted cocoa butter, or any other fluid) and a solid (the lungs) of a less specific gravity, can any one doubt that, whatever the position of the body of the animal, the lighter body, the lungs, like the bit of cork in the bottle, will rise as high as the surrounding walls or its attachments will permit?

These appear to me the fundamental errors in Dr. Garland's theory; those of you who read his work will admit that I have not touched upon many minor ones. The facts that the dean of our medical school has stated that Dr. Garland's ideas are to be demanded of the students in their examinations,[1] and that they have been presented here without being discussed, are my apology for occupying so much of your time this evening.

On page 57 of his book Dr. Garland speaks as follows:—

"No further evidence can be needed to prove that the results which Ferber obtained are identical with those which I obtained and have described. Ferber's thoughts, however, seem to be riveted upon one idea, namely, that the body of the injection seeks and occupies the lowest parts of the chest, and he utterly fails to appreciate the significance of the upper part of a fluid being maintained by an invisible support in a condition of hydrostatic inequilibrium."

[1] The dean of the medical school said at a subsequent meeting that this statement on the part of Dr. Hunt was the result of a misapprehension of the facts.

Need I say that in the light of the facts here presented Dr. Ferber interprets the phenomena according to natural laws, and that Dr. Garland's "thoughts seem to be riveted" to an imaginary but impossible hydrostatic level to such an extent that he cannot recognize the important action of that force which keeps us all from flying off at a tangent into chaos?

THE DIPHTHERIA QUESTION.

THE arguments in favor of considering "membranous croup" and diphtheria identical diseases have been forcibly stated by Dr. Curtis. Dr. Seitz,[1] after an active practice of forty years, in a city where diphtheria is almost endemic, has been led from his experience in the polyclinic and his observation of several hundred cases, beside having spent some time in studying the disease in Paris in 1867, in Vienna in 1875, and in various other places at different times, to agree with Jaccoud, Oppolzer, and the distinguished epidemiologist Hirsch, that the one is a purely local malady and the other a constitutional disease. He also quotes Virchow, as we have done, in support of his position that there is an anatomic pathological difference between the two morbid processes. We do not care just now, however, to take the time of our readers in discussing the question of the identity of the two diseases, or in arguing whether or not pathological distinctions should be an important guide in the nosological arrangement of diseases. While acknowledging that the relationship between diphtheria and croup is far from being satisfactorily elucidated, and that a sharp dividing line between them cannot always be established, it seems to us that the balance of opinion among men who have large opportunities of observation is decidedly with Seitz. Virchow, for instance, whose views on the pathological side of the question have already been referred to, in reviewing the mortality of Berlin, speaks of "croup, diphtheria, diarrhœa, measles, scarlet fever, puerperal fever, etc.," as the diseases that have steadily increased in prevalence and fatality; this he hardly would have done, if he considered croup and diphtheria identical.

Among the health boards reporting diptheria and croup under different heads are those of Glasgow, Dublin, Belfast, Brussels, Pesth, Vienna, Paris, Naples, Berlin, Breslau, Amsterdam, Copenhagen, St. Petersburg, Alexandria, Milan, and throughout England; in this country that practice is almost universal, if not quite so. Dr. Farr, in his last report, speaks of diphtheria as having formerly been confounded with croup. Hamburg, Dresden, Munich, Rome, Turin, Venice, are the only cities which we can find on our lists where the two diseases are reported together; typhus and typhoid fevers are still returned in a similar manner in some places.

In England diphtheria is now commonly classed as a filth-disease; croup is not. The admirable state board of health of Holland require, in all cases, the following infectious diseases to be reported: small-pox, varioloid, scarlet fever, measles, typhus fever, typhoid fever, cholera, diphtheria, and dysentery; not

[1] Bibliothek für Wissenschaft und Literatur, 19 Band. Medicinische Abtheilung. 3 Band. Diphtherie und Croup, geschichtlich und klinisch dargestellt von Dr. Franz Seitz, ord. Prof. der Med. an der Universität München. s. 516. Berlin, 1877.

croup. In Brussels, where sanitary science has advanced to a point unparalleled in this country or in England, laws regulating the spread of infectious diseases have existed since 1818; and by the latest circular of the Bureau d'Hygiène (December 31, 1877) physicians are advised to report all their cases of small-pox, scarlet fever, measles, typhoid fever, typhus fever, cholera, diphtheria, and epidemic dysentery; not croup. An order has just been passed in Vienna requiring, in diphtheria, isolation of the sick, thorough disinfection of the houses, removal of the patients to the hospital, if they have not room at home, transportation of the corpses at once to the dead houses, free distribution of disinfectants to the poor, and *daily* reports of the progress of the disease to the city physician; croup is not included in the order.

If the Boston Board of Health believe, as they have pretty good authority for doing, that typhus and typhoid fevers, diarrhœa and dysentery, as also croup and diphtheria, are distinct diseases, of which, in our city, typhus fever and diphtheria alone are so dangerous to the public health as to require interference on their part, will they not do a better work in the end, and are they not more likely to meet hearty coöperation from physicians and success in their plans, if they make their requirements correspond with their belief, although the above-named diseases are often confounded? In this connection we wish to repeat our statement that diphtheria should be treated with the utmost precaution, so far as steps for preventing contagion are concerned. The Board of Health would be justified, in our opinion, in much stronger measures than any they have yet taken, even to requiring immediate burials after death, and to forbidding public funerals in so grave a disease. Such extreme measures have been taken elsewhere, but we do not know of a single board of health where "membranous croup" has been included with diphtheria in any regulations for restricting infection. There is no evidence known to us that diphtheria is ever transmitted from a person suffering with "membranous croup." The very opposite is true of severe and mild cases of scarlet fever, which have been cited as analogous to diphtheria and croup.

MEDICAL NOTES.

— The attempted cultivation of the cinchona-tree in Australia is, from climatic reasons, a complete failure.

— The dean of the Paris Faculty of Medicine states that but thirty-five women have studied medicine in that city since 1865, of whom nine have diplomas, and twenty-three are still studying. Of the latter six are English, twelve Russian, and five French.

— The death-rate in St. Petersburg for the last week in January was forty-six per one thousand, higher even than the Bombay rate.

— Dr. Wilson Fox, having been informed by the council of the British Medical Association that women cannot be excluded from the meetings of the association, has withdrawn his name from the list of members, and others are likely to follow his example.

— Italy, as yet, has no pharmacopœia; a compilation by Professor Orasi

having been generally employed. The *American Journal of Pharmacy*, quoting the *Pharm. Zeitung*, states that a commission for the preparation of the needed work has been appointed by the government, and was organized at Rome in October, 1877, Senator Connizaro, professor of chemistry in the University of Rome, being the president.

— Dr. Lievin, in his annual report, shows that the death-rate of Dantzic for the years 1863–69 was 36.85 per 1000; 1870–71, 36.33; 1872, 31.27; 1873, 26.25; 1874, 24.99; 1875, 30.44; 1876, 28.67; 1877, 28.10. There has been an improvement in the public health from the introduction of sewerage and a pure water supply, but not so great as in England. The mortality from phthisis has steadily increased from 20.5 per 10,000 in 1863–69; 25.0 in 1870–75, and 25.41 in 1876 to 27.21 in 1877, in spite of the drying of the soil by the sewers. The death-rates for single streets, blocks, etc., reached the enormous figures (per 1000) of 68.5, 73.2, 65.8, 70.0, 70.3, 75.2, and 63.9. In individual houses even one tenth of the occupants died within the year.

— The twenty universities of Northern Germany cost the country annually about two millions of dollars. The University of Leipsic alone receives two hundred and fifty thousand dollars. These twenty universities have a staff of twelve hundred and fifty professors, who receive salaries varying from five hundred dollars to three thousand dollars. The young man who embraces the career of teaching can calculate on having a salary of two thousand dollars when he reaches the age of thirty-five. He is certain also of a pension when retired. Germany has a university for every two millions of inhabitants, Austria one for five millions, England one for seven millions, and Switzerland one for one million.

— With the completion of the new fever hospital at Belvidere, lately dedicated, Glasgow is probably the best supplied with hospitals for infectious diseases of all the cities in the world. The hospital treatment for such maladies began there in 1865, in a series of airy, temporary buildings, under the able direction of Dr. J. B. Russell, now medical officer of health for the city. Unlike the English custom, which places fever hospitals under the Poor Law Board, the Glasgow system is to have them controlled by the sanitary authorities. The new buildings have entirely separate departments for the different infectious diseases, and suitable accommodations are made for private patients as well as for the pauper class. The floors are of Dantzic oak, waxed; the walls are finished with Keene's cement, as being considered the least liable to retain infective material. In six years the inspectors of the Board of Health have made 1,366,708 house-to-house visitations, thereby discovering 14,200 cases of infectious disease, which are never lost sight of until removed to the hospital, or, when circumstances admit of their being treated at home, until convalescence. The result, largely due to this thorough treatment, but partly, of course, arising from better general sanitary conditions, is that, from typhus fever alone, there were 3994 deaths in the five years previous to the establishment of the hospital, estimated to represent 33,300 cases, with an average population of 412,500; and that in the five years ending with 1876 there were only 553 deaths, estimated to correspond to 4600 cases, with a population of 520,000. Here is a saving of 4450 lives, and 37,400 cases of sickness from one disease in five years.

— In France, notwithstanding so many persons believe that wine-drinking prevents drunkenness, they have seen the need of passing the following law: That every one condemned twice by the police for the crime of open drunkenness is held to be incapable of voting, of elective eligibility, and of being named for the jury or any public offices.

— The *Medical Brief* says: It has been noticed in several cases that when one quarter of a grain of morphine would not produce sleep, if ten grains of quinine were administered a short time previous to administering the morphine, the morphine would almost invariably act efficiently. This fact was noticed in connection with puerperal cases.

DR. JOHN E. TYLER.

"DIED, at his residence in Boston, on March 9th, John Eugene Tyler, M. D., aged fifty-eight years."

Dr. John E. Tyler was born in Boston on the 9th of December, 1819. He was the second son of John E. and Hannah Parkman Tyler, of Westborough. His father, a graduate of Harvard in 1786, was educated a physician, and practiced his profession for some years in Westborough, but afterwards became engaged in business in Boston. Dr. Tyler, the subject of this notice, was himself early destined to a mercantile life; but though he developed an aptitude for business, which served him a good turn in hospital management later, he cherished a similar repugnance to that of Charles Lamb for the "desk's dull wood," and soon quitted the servitude of the counting-room for the more congenial realm of study.

His preliminary education was begun under the auspices of Rev. Dr. Kittredge, in Westborough, and continued at Leicester and Phillips (Andover) academies. He entered the freshman class of Dartmouth College in 1838, and graduated in due course and with high honors in 1842. This class numbered one hundred and one, being the largest as it was in other respects one of the most noted in the history of the college. Here Tyler gave evidence of that ready wit and humor which has always been a conspicuous element in his nature, and which, added to brilliant scholarship, gained for him an immense popularity in his class. Here, too, shone forth those varied accomplishments known only to his most intimate friends in later years. He was foremost in all athletic games and sports, — running, leaping, skating, swimming, and the like. He was a fine musician in the best acceptation of that term, a singer, and an adept upon several different instruments. During his senior year he was president of the Handel Society, a college institution almost coeval with the beginning of the present century, and of no mean pretensions even when judged by the high standard of the choral societies of the present day. He was also a good writer, and an easy and graceful speaker, and on this account, conjoined with his rank in scholarship and general popularity, was made president of the United Fraternity, one of the two leading literary societies of the college. He was also a member of the Phi Beta Kappa and Psi Upsilon Chapters, access to which in those days was gained only by merit.

Almost immediately after his graduation from college he went to Newport,

R. I., where he taught school for a short time, and then entered upon the study of his chosen profession, under the guidance of the late Dr. Dunn, of that city. He subsequently attended a course of medical lectures at Hanover, and two sessions at the medical department of the University of Pennsylvania, in Philadelphia, at which latter institution he graduated in the spring of 1846. He also received a medical diploma at Hanover. As a student of medicine he was most assiduous at his tasks, and attracted the favorable notice of the professors. At the final examination for a degree in the university the strictness of the ordeal was often in proportion to the presupposed fitness of the candidate. If his record was a good one, the student was let off easy; if otherwise, he was ground as between the upper and nether millstone. In Tyler's case, when he came before the venerable Dr. Chapman, he was asked the two following questions: (1.) "*What are tormina?*" This being answered satisfactorily, (2.) "You have studied with my old friend, Dr. Dunn, of Newport. *Are there as many pretty girls there now as there were forty years ago, think you?*" This being answered in the affirmative, for aught he knew to the contrary, "That 's enough," said the professor; "I shall vote for you with all my heart."

Dr. Tyler now entered upon the practice of his profession at Salmon Falls, in New Hampshire. While there he was sent to the state legislature, and was soon called to take charge of the New Hampshire Asylum for the Insane, at Concord, in which position he remained some five years, when he was appointed to the honored post of physician and superintendent of the McLean Asylum for the Insane, at Somerville, made vacant by the resignation of Dr. Bell. This was in 1858. Here he remained till the spring of 1871, when he was compelled by failing health to offer his resignation, which, after much delay and with great reluctance, was accepted by the trustees. It was during this long term of service at Somerville that Dr. Tyler showed that marked executive ability, sound judgment, knowledge, and skill which have made his name famous in this and in other countries. His official reports while at the head of the McLean Asylum have been largely quoted, and are recognized by the profession as among the ablest and best in this department of medical literature.

Dr. Tyler twice visited Europe, where he enlarged and enriched his knowledge of his favorite science, and was received by his confrères in the psychological associations of Great Britain and Ireland with marked courtesy and attention. Upon his retirement from hospital life he took up his residence in Boston, where he soon acquired a large consulting practice in his specialty. In 1871 he was appointed to the chair of mental diseases in the medical department of Harvard University, having previously been connected with the Medical School as university lecturer on the same subjects. He was a fluent and graceful lecturer, — always popular with his students.

In recent years Dr. Tyler has held several important posts in connection with our city and state commissions. He was a member of the American Academy of Arts and Sciences, also of the Boston Society for Medical Improvement, and several other associations for professional advancement, and was accustomed to mingle freely in their discussions. He was also a trustee, under the will of the late Seth Adams, of the proposed institution for the treatment of nervous diseases. In all these capacities he was unsparing of his own

powers, faithful, most conscientious in the discharge of every duty, always ready to go at the beck and bidding of others at whatever cost or sacrifice to himself. Here, indeed, we find one of the prominent traits of his noble and unselfish nature. He made himself prematurely old in his unremitting labors for the cause of science and of humanity. During the last winter especially he felt conscious of failing strength, and of the onset of serious if not fatal disease. But no warnings of friends or forebodings in his own mind could suffice to hold him back from duty. He kept his armor on, and bright with active service till the last; and when the summons came, in a form which he well knew in his case to be the final call, he accepted it, cheerfully and with resignation, almost with gladness, — literally

> "Like one that wraps the drapery of his couch
> About him, and lies down to pleasant dreams." U.

"OBSCURE FORMS OF LIVER DISEASE."

Mr. Editor, — In the account of the proceedings of the Hampden District Medical Society [1] it is stated that, after the paper on Obscure Forms of Liver Disease was read, it "was briefly discussed." Allow me to present an imaginary discussion, which in the minds of not a few of the intelligent members of the society must doubtless have been carried on, although the record unfortunately fails to give the details.

Critic. What proof have we that the liver was affected in the first case? On the contrary, there are positive proofs of renal difficulties of the severest type, — pus, blood, casts, painful micturition, severe pain in the right renal region, and tenderness in both renal regions. Only when that pain was the severest was there icterus, apparently a consequence of something extending to the liver, at these times. But there was no tenderness or enlargement of the liver. Now, the question arises whether a yellow skin and some light-colored dejections (intermittent according to the above-named pain) ought to lead us to suspect hepatic disease as the chief trouble. I think not.

But what have we? Owing to the imperfect report we cannot definitely say; but the fact that the first physician found enlargement *behind* the right hypochondriac region leads me to ask whether a nephritic or perinephritic abscess may not explain the whole matter. The pain and swelling in the back, the paroxysms of suffering for three months in the same parts, and none at all found in the hepatic region in front, the urinary signs, all point in the direction of the kidneys. The other symptoms do not contraindicate this hypothesi Should not the swelling in the back have been explored by the aspirator? Was there any distinct tumor felt between the front wall of the abdomen *below the line of the umbilicus* and the renal region? We do not have any information about these questions, yet, if the palpation indicated had been made, it might have helped the diagnosis. Thus, whilst we are quite unable, owing to the imperfect record of the case, to come to a clear diagnosis, the symptoms undoubtedly point to the urinary rather than to the hepatic organs as the seat of the primary disease. The jaundice and purpura may readily have followed as

[1] Journal, March 7, 1878.

consequences either of local pressure or of general cachexia resulting from the local disease, while syncope, palpitation, and dyspnœa are among the very common accompaniments of renal trouble. Finally, considering that the case is brought forward as being one of hepatic disease, while, at the same time, thoracic symptoms are occurring, one may ask, Why are we not informed of the results of auscultation of the heart and lungs? The critic contended that this was a serious omission, and he concluded in the following words: Case I. not proven to be hepatic disease at all, but the evidence is greatly in favor of its having been renal.

The records of the secretary of the society do not give any reply to the critic, although doubtless one was given, mentally at least, and I sincerely wish we could see it.

The second case also was still more certainly, in the mind of the critic, not hepatic originally. He had also the boldness to assert that, even with the imperfect narrative of the details of the case, he would take the ground that it was a simple case of neglected pleurisy, from the first. Proper precautions were not used, and auscultation in the earlier period of the disease was not made. Hence, by allowing the patient to go out to his work "though not fully recovered," empyema followed, with an opening in the fifth intercostal space. The pain and tenderness in the hepatic region would be very easily explained by a large effusion pressing down the liver. The fact that the patient was able to go to work, "though not fully recovered," is entirely consistent with the possibility of the right cavity of the chest being, at that time, full of fluid. But we really knew nothing of the facts as to pleuritic effusion, till he came down again with increase of trouble. Possibly aspiration would have cured at the earlier period. Hence, continued the critic, the final questions submitted by the reporter may be answered thus: —

First. The pus came from the pleural sac, and probably from that only; there is not a particle of proof that it came from the liver.

Second. There is no proof of any disease of the liver in either of the cases. The critic still further remarked that the idea of hepatic disease would never have suggested itself to him, even with the imperfect record given by the writer.

Third. There was no "abscess elsewhere," in the second case, but a simple empyema, threatening from the time of the earliest symptoms, and culminating when the opening occurred.

In conclusion, the critic claimed that gentlemen when reporting cases should always give not only the positive symptoms and signs of disease, but should also put aside, by accurate record of negative facts, the possibility of the existence of other and allied diseases, with which the case recorded might be confounded. Especially should this be done when cases are presented to the reporters for medical journals.

I trust, Mr. Editor, in thus giving you the above imaginary criticisms upon two cases reported from the Hampden District Society, that our friends will not deem them improper. We all wish to arrive at truth. That can be gained only by the clash of opinions upon well-recorded facts.

Yours truly, MEDICUS.

COMPARATIVE MORTALITY-RATES.

	Estimated Population, July 1, 1878.	Deaths during week ending March 2, 1878.	Annual Death-Rates per 1000 living.		
			For the Week.	For the Year 1877.	Mean of ten Years, '68-'77.
New York.	1,093,171	522	24.83	24.32	28.71
Philadelphia.	876,118	312	18.46	18.80	21.54
Brooklyn.	549,438	193	18.27	21.51	25.50
Chicago.	460,000	113	12.77	17.83	22.39
Boston.	375,476	128	17.73	20.10	24.34
Providence.	104,500	47	24.44	18.81	19.20
Lowell.	55,798			19.09	22.50
Worcester.	54,937	11	10.32	14.07	22.30
Cambridge.	53,547			18.69	20.83
Fall River.	53,207	18	17.59	21.35	24.96
Lynn.	35,528	15	21.96	20.42	19.67
Springfield.	33,981	6	9.19	16.04	19.77
Salem.	27,140	13	22.99	20.38	21.15

BOSTON SOCIETY FOR MEDICAL OBSERVATION. — At a meeting of this society, to be held on Monday evening next at eight o'clock, at its rooms, 36 Temple Place, Dr. T. B. Curtis will read a paper upon Cases of Lithotrity.

MASSACHUSETTS COLLEGE OF PHARMACY. — At the annual meeting, held March 4, 1878, the following-named gentlemen were elected officers for the ensuing year: President, Samuel A. D. Sheppard. Vice-Presidents, Thomas L. Jenks, M. D., William S. Folger. Recording Secretary, D. G. Wilkins. Corresponding Secretary, George F. H. Markoe. Treasurer, Charles I. Eaton. Auditor, James S. Melvin. Trustees, Benjamin F. Stacy, S. C. Tozzer, Charles P. Orne, Edward S. Kelley, I. B. Patten, George H. Cowdin, Edgar L. Patch. Secretary of the Board of Trustees, Henry Canning. The college and the school of pharmacy were reported to be in very satisfactory condition.

G. F. H. MARKOE,
Corresponding Secretary.

MIDDLESEX (EAST) DISTRICT MEDICAL SOCIETY. — The next meeting of the society will be held with Dr. Wight, at the Central House, Woburn, Wednesday evening, March 20th, at 7.30 o'clock.

I. RICHMOND BARSS, *Secretary.*

APPOINTMENT. — Dr. Edward Wigglesworth has been chosen physician to out-patient department for skin diseases at the Boston City Hospital.

BOOKS AND PAMPHLETS RECEIVED. — Baths and their Uses in the Treatment of Diseases of the Skin. Valedictory Address to the Class on Diseases of the Skin at the Philadelphia School of Anatomy and Surgery, January 16th. By John V. Shoemaker, A. M., M. D. Philadelphia. 1878.

The Future of Sanitary Science. An Address delivered before the Sanitary Institute of Great Britain at the Royal Institution on July 5, 1877. By Benjamin Ward Richardson, M. D., LL. D., F. R. S. London: Macmillan & Co. 1878. Pp. 47. (For sale by A. Williams & Co.)

On the So-Called Eczema Marginatum of Hebra as observed in America. A Clinical Study. By L. Duncan Bulkley, A. M., M. D. Read at the Annual Meeting of the American Dermatological Association. (Reprinted from the Chicago Medical Journal and Examiner.) New York: G. P. Putnam's Sons. 1877.

Are Eczema and Psoriasis Local Diseases of the Skin? By L. Duncan Bulkley, M. D., Philadelphia. 1877.

On the Recognition and Management of the Gouty State in Diseases of the Skin. By L. Duncan Bulkley, M. D.

Annual Announcement of the Medical College of the Pacific Session of 1878.